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INTRODUCTION TO MR WHATELY CARINGTON'S AND  
MR SOAL'S PAPERS

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THE Editor has asked me to write a brief introduction to the two papers, by Mr Whately Carington and Mr Soal, which form the main contents of the present number of the S.P.R. *Proceedings*. I am very glad to do so. Mr Whately Carington's work has been done in Cambridge in consultation with a committee composed of himself, Mr Gatty, Dr Irwin, Dr Thouless, and the present writer. This committee has held its meetings in my rooms in Trinity ; so, although my lack of expert knowledge and practical experience in statistical method has prevented me from contributing anything of importance to their debates, I have at least been in constant touch with this investigation from the outset. I have had no such direct contact with Mr Soal's work. But I have read all that he has published on the subject with great interest, and I have always admired the patience, thoroughness, and accuracy of all his many-sided contributions to psychical research. It is an ill war that blows no one any good, and the disaster that has befallen Europe has at least had the good result of making Mr Soal a temporary resident in Cambridge, where the members of our committee live.

I suspect that the first reaction of many members of the Society on opening this number of the *Proceedings* will be to cry "Another mass of boring statistical stuff!", and that the second reaction of some of these will be to leave the contents unread. This kind of behaviour may be natural, but it is foolish on general grounds and it would be most unfortunate in this particular case. I will now give my reasons for these two assertions.

In almost every branch of psychical research the first question that arises is this :—Have coincidences of a certain kind happened much more often than they might reasonably be expected to do by chance? This question crops up in investigating mediumistic communications which purport to give information about a dead person, and in investigating the claim that a house is haunted, just as much as it does in experimental researches on alleged para-normal cognition, such as are reported in the present number of *Proceedings*. In the first case we want to know, before going any further, whether considerably more of the medium's statements about the alleged communicator agree with the facts about him than might reasonably have been expected by chance. In the second case we want to know whether coincidences between staying in this house and having sensory hallucinations of a certain kind are more numerous than we might reasonably have anticipated if chance alone were operating. It is only when these questions have been answered in the affirmative that there is anything worth investigating further.

But, although this kind of question arises everywhere, and although in many cases we understand vaguely what it means and we have a stronger or weaker personal conviction as to the answer, it is only where statistical methods can be applied that the question becomes precise and that a completely definite and objective answer becomes possible. What exactly is a "coincidence"? What is meant by a coincidence "happening by chance"? How often might coincidences of a given kind be reasonably expected to happen by chance? What amount of deviation from the most probable chance-frequency would it be reasonable to ascribe to chance? There is one field of human activity in which precise answers can be given to all these questions, and it is one that is perfectly familiar to all of us, viz. what we call "games of chance", such as are played with cards, dice, roulette-boards, and so on. These answers can be carried over into other fields without loss of relevance or intelligibility just in proportion as those other fields are analogous in all relevant respects to actual or conceivable games of chance.

Let us take an example from ordinary playing-cards. No one competent to express an opinion would question, e.g., any of the following statements, except on the ground that a mistake might have been made in the calculations which led to them. The most likely number of hearts in a whist-hand properly dealt from a properly constructed and shuffled pack of ordinary playing cards is 3. The probability that such a hand will contain exactly that number of hearts is about .286. The probability that it will contain exactly 4 is only slightly less, viz. .238. The probability that it will

contain more than 6 hearts is .0112. Suppose that 1000 hands were dealt. Then the number of them which contain more than 6 hearts apiece is more likely to be 11 than any other number. The probability that it will be exactly 11 is not great; but the probability that it will be less than 16 is .9332, and the probability that it will be less than 20 is .9965. Therefore, if in a thousand hands there were considerably more than 20 which contained more than 6 hearts apiece, it would be reasonable to feel almost certain that there was something wrong with the pack or the shuffling or the dealing.

Now contrast this example with cases where statistical methods cannot be applied, and one can only make such remarks as "It is incredible to me that Mrs X, that simple and ignorant woman, should by chance have made so many true and striking statements about the deceased Mr Y". Possibly one's incredulity may be quite justified, and possibly the situation may have been such that a majority of fair-minded readers with no strong prejudices against mediumistic communications will come to share it. But it is all terribly personal and subjective, and experience shows that it leaves most plain men and scientists completely unmoved.

For such reasons as these I believe that experiments in psychical research which are capable of precise statistical treatment are of the utmost importance. I do not think that we shall ever get orthodox experimental psychologists to attend to our work unless and until we can produce results of this kind. We must remember that experimental psychology is very much in the position of a woman with a shady past who has at length, after a hard struggle, settled down to a respectable life and got on visiting terms with the doctor's, the solicitor's, the vicar's, and even the squire's wife. (The scientific equivalent of this apotheosis is being admitted to form a section at the British Association.) She is fanatically determined to keep her hard-won respectability unsullied by the slightest breath of scandal. Physics, which has been honoured for centuries, can afford, like the scion of some noble house, to throw her cap over the mills; but poor dear psychology feels that she dare not take risks. Now the work of contemporary orthodox experimental psychologists is very largely statistical, as anyone can see who looks at their journals. Since this ground is so very familiar to them, there is a hope that a few of those extremely shy birds may be tempted to hop over the wall which separates them from us, if we can offer them some statistical crumbs, of unimpeachable wholesomeness, to peck at. They certainly ought to be investigating the problems which interest us, instead of ignoring them or unhelpfully nagging at us for the crudity and amateurishness of the methods which we have to use.

in default of their advice and assistance. But I am sure that they will continue to neglect that duty until we can bring to their notice properly conducted researches of a statistical kind, such as those of Mr Whately Carington and Mr Soal.

There are two remarks of an historical kind which it may be worth while to make before we leave generalities. The first is this. Some readers may be tempted to complain that such researches as these can at best lead only to trivial and pedestrian results. What interests them, and what induced them to join the S.P.R., was the hope of finding answers to questions of fundamental human importance, such as the survival or extinction of the individual and the destiny of the race. Investigation of trance-mediumship, they will say, has at least a chance of throwing light on these important questions ; but these experiments on drawing pictures and guessing cards are at best *difficiles nugae*.

I think that the history of science shows this attitude to be entirely mistaken. It is precisely the attitude against which Galileo, Descartes, Bacon, and other great men who assisted at the birth of modern science, constantly and energetically protested. What could seem more trivial than Galileo dropping balls down inclined planes and noting the times of their descent? Yet those experiments are the basis of the science of dynamics, and without them Newton's all-embracing theory of the planetary system would have been impossible. Similarly, if we could establish the fact and disentangle the laws of extra-sensory perception by experiments on ordinary persons in artificially simplified situations, we might hope eventually to form comprehensive and satisfactory theories about mediumistic communications. But, if we insist on delivering blind frontal attacks on unanalysed problems of immense complexity, we are likely to share the fate of the scholastic physicists.

The second point is this. Any science starts by consisting mainly of "natural history". If it develops, the proportion of natural history becomes smaller and the proportion of theory and deduction becomes greater. As this happens, the science grows more technical, and it becomes more and more difficult for the interested amateur to contribute anything of value to it, or even to understand the contributions of experts. Just contrast, e.g., the early meetings and the early *Transactions* of the Royal Society with those of the present day. If we may compare small things with great, we must expect that something of this kind will happen with the S.P.R. unless it be engaged on a wild-goose chase. Undoubtedly there is still an immense field for "natural history" in our subject, and for many years to come the intelligent and curious amateur will be able

to make most valuable factual contributions to psychical research. But already, in my opinion, the investigation of alleged physical phenomena in the séance-room has ceased to be a job for anyone but a trained specialist with an elaborate knowledge of electrical and photographic technique. I am equally convinced that we have now reached a stage in the study of para-normal cognition under experimental conditions at which statistical methods must be constantly and increasingly used. Members of the S.P.R. would do well to reconcile themselves to this inevitable tendency, and to prepare themselves for it by making that quite moderate study of the elements of statistics which would enable them to follow intelligently the reasoning contained in such papers as Mr Whately Carington's and Mr Soal's. They would find that the effort was rewarded, not only in connexion with psychical research, but also in the many other departments of life in which statistical concepts and methods are now applied.

I hope that I have now shown adequate cause why the two papers which follow should not be ignored off-hand on the general ground that they contain a good deal of statistics and that the results which they establish make little direct appeal to our higher emotions and aspirations. It remains for me to state some positive reasons why they deserve to be read with special care and attention.

In the first place, both sets of experiments have been conducted with a degree of care and thoroughness which has probably not been equalled and has certainly not been surpassed in any previous work on the subject. The writers have been at pains to exclude every possible kind of normal "leakage". They have stated exactly what precautions they took, and have given so clear and full an account of the conditions under which the experiments were performed that anyone who chooses can repeat them exactly. A critic who wishes to show that there was opportunity for normal leakage will have to indicate some specific defect in some recorded detail of the technique ; he will not be able to base his criticisms (as in many cases he quite justifiably can) on the negative ground that "we are not told whether the percipient had such and such chances of acquiring knowledge normally". There may have been other experiments on para-normal cognition in which the conditions were *in fact* as rigid as they were in these ; but I do not think that there have been any in which *we know* them to have been so rigid as *we know* them to have been here.

It is important to notice that Mr Whately Carington has devised a kind of experiment which ought to give *positive* results, if repeated, in a *fair proportion* of the repetitions. Of course something may

depend on the mental attitude of the agent or transmitter, but there should be no insuperable difficulty in finding suitable agents who are sufficiently interested and sympathetic. To have achieved this result is a real step forward. Provided that later reflexion and discussion do not reveal unforeseen sources of error, Mr Carington has (probably for the first time in the history of the subject) produced a *repeatable* experiment.

Secondly, it is most unlikely that there is any flaw in the statistical technique. Objections on this ground have been brought by certain statistical pundits in America against Dr Rhine's results, though Mr Soal and Dr Thouless have shown that these objections are in the main captious and nugatory. In the present case it is doubtful whether even the most captious statistical critic can have much to say. In Mr Soal's experiments the statistical problem is perfectly simple and straightforward, and it does not differ essentially from that of Dr Rhine's experiments. By carefully randomising the cards in the way which he describes, and by using Mr Stevens's formula, which allows for the actual preferences of the guessers among the five kinds of Zener cards, Mr Soal has obviated the only criticisms on Dr Rhine's statistical assumptions which ever had any substance.

A careless reader, on a first hasty reading, might be inclined to think that Mr Whately Carington's "method of palpable hits" is open to criticism because it makes use of the opinion of a certain individual judge as to whether such and such a drawing does or does not resemble such and such an original enough to be counted as a "palpable hit" on the latter. This criticism is fallacious, as Mr Carington has most carefully, lucidly, and conclusively shown in his paper. I will not discuss the matter further here, because I have tried to explain the statistical situation by means of an analogy which Mr Carington has embodied in an appendix. It will suffice to say that, if there be any statistical fallacy in Mr Whately Carington's paper, it has eluded, not only myself (a very feeble defence), and not only Dr Thouless and Mr Gatty (who have constantly to use and to appraise statistical reasoning in the course of their psychological and biological work), but also Dr Irwin, who is an expert professional statistician.

A third merit of these experiments is that we know that we have *all* the results, good, bad, and indifferent, before us. There is no room for the suspicion which attaches, rightly or wrongly, to some long series of experiments on para-normal cognition, viz. that the experimenter began to record his results only after they began to be exciting, that he stopped recording them when they ceased to be so,

and that he discarded results which were got when the subject was "off-colour".

The fourth reason for attending carefully to these two sets of experiments is that they led to positive results of a high degree of statistical significance. Consider, e.g., Mr Whately Carington's answer to the question whether or not his percipients tend, to a significant extent, to score relatively more hits on the originals of the experiment in which they are engaged than they do on those of experiments in which they are not engaged. He finds, on the most conservative method of scoring, that the actual deviation from the most probable number of successes on the hypothesis of chance is positive and is 3.572 times the standard deviation. What precisely does this mean? Suppose we compare the whole of Mr Carington's material in all these experiments to a single "deal" or "throw" in a game of chance which is fairly played with properly constructed apparatus. Suppose we imagine a similar set of experiments to be repeated, with the same amount of material on each occasion; and suppose we compare each such repetition to a new "deal" or "throw" in the same game of chance. Suppose that the positive results which Mr Carington actually got were like some big chance deviation from the normal in a single deal, e.g. holding 10 or more hearts in a fairly dealt bridge hand. Then one could not reasonably expect to get so great a deviation more than once in 1000 such "deals", i.e. in 1000 repetitions of such an experiment as is here described.

Now take, e.g., Mr Soal's figures for the successes scored by his subject Mrs S. on the actual card, the one immediately before it, and the one immediately after it. The divergences of these from the most probable numbers were all positive, and were respectively 2.627, 3.309, and 4.164 times the relevant standard deviations. The meaning of these statements can be interpreted as above. The actual numbers concerned will be different; in the first case somewhat less than 1000, and in the third case very much more than 1000, repetitions of the experiment would be needed before it would be reasonable to expect so great a deviation. Moreover we have to take into account the further fact that all these large deviations are *positive*, whereas in a game of chance, they might just as well have been negative; and that they are *clustered about* the card actually aimed at by the percipient.

I will now say something about the connexions and the disconnexions between the two papers. The two investigations began and continued for a long time in complete independence of each other. Mr Soal worked for five years with Zener cards and individual

percipients in London. Mr Whately Carington has been working for about a year and a half with drawings and groups of percipients. The drawings have been exhibited in Cambridge, the groups of percipients have been in various places. It was not until November 1939 that the two sets of experiments converged. Mr Carington had found a highly significant degree of post-cognitive and pre-cognitive success among his subjects, and he suggested to Mr Soal that the latter should look through his results and compare the guesses made by his subjects with the cards exposed immediately before and immediately after the card at which the guess was deliberately aimed. The remarkable outcome of this comparison, in the case of two of his subjects, forms the content of Part II of Mr Soal's paper. It certainly adds to the dramatic interest of these two investigations that a suggestion, based on the results of one of them, should have revealed that the other had led to a highly significant positive result which would otherwise have been overlooked.

The following two points of difference should be noted. (i) Mr Whately Carington is led by his experiments to conclude that the power of para-normal cognition is *widely distributed* but *very weak*, so far as concerns the subjects whom he has tested and the tasks which he has assigned to them. Among Mr Soal's subjects, who are engaged in a very different kind of activity, the situation seems to be quite different. When the scores were re-investigated for signs of pre-cognitive and post-cognitive knowledge only two of his subjects were found to have shown it to any appreciable extent. These two seem to possess it strongly; and they are also outstanding, at any rate in their earlier scores, at guessing contemporarily exposed cards. (We must not, at this stage, ignore the possibility that, if the guesses made by the other subjects were tested for successes on more remote cards than the three central ones, they might be found to have scored significantly.) (ii) As Mr Soal has pointed out, the guesses which he has called "pre-cognitive" need not have been so in the sense of involving present knowledge of a future event. The cards which will be turned up after a given guess has been made already exist and are already on the table covered up by other cards. If the subject can clairvoyantly cognise cards which have not yet been exposed in the course of the experiment but are already in the pack, he will be able to make guesses which are "pre-cognitive" in the sense required by Mr Soal's results. Now this is not so with the pre-cognitive knowledge which appears to be manifested in Mr Whately Carington's experiments. This is knowledge of a drawing which has not yet been made, a drawing whose subject has not yet been chosen.

In this connexion the two following remarks are worth making. (i) Dr Thouless, who had done a number of experiments on guessing Zener cards with students at Glasgow and had got entirely chance results, was encouraged to look over his records for post-cognitive and pre-cognitive successes. He found that there was no trace in them of significantly non-chance scores. This is exactly what Mr Soal would have found if he had not been lucky enough to include among his 160 subjects those two white blackbirds Mrs S. and Mr B. S. (ii) Mr Soal found no significant evidence for pure clairvoyance, either of the card at which the subject was aiming or of cards which came immediately above or below it in the pack, in the experiments which were specially directed to this question. He has not as yet had time to make elaborate counts for more remotely displaced successes. Of course it is possible that Mrs S. and Mr B. S. would have been exceptions, for in fact they tried only undifferentiated extra-sensory perception. If we suppose that they would have failed at pure clairvoyance, as did all the subjects who tried it, we shall have to hold that their pre-cognitive successes really did involve foreseeing what the agent was going to see when he afterwards turned up the next card, and that they did not just clairvoyantly perceive that card while it was still lying covered on the table. In that case their pre-cognitive results would be in line with those of Mr Whately Carington's subjects.